

## Secondary Sources in Parapsychological Research: A Vicious Cycle

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Having taught courses in experimental parapsychology on an annual basis since 1980, and having published several articles on the topic of computerized testing of extrasensory perception (ESP; Vitulli, 1982, 1983; Vitulli, Cain, & Broome, 1985), I was elated to read Child's (November 1985) effort to "turn psychologists around" with respect to prejudice toward psi research in general and prejudice toward research into the possibility of ESP in dreams in particular.

The secondary sources that surveyed research on ESP and other anomalous experiences referred to by Child (e.g., Hansel, 1980; Zusne & Jones, 1982) are surely not unique in kind to the field of parapsychology. Textbooks in general psychology, experimental psychology, personality, and social psychology, for example, tend to slant material toward the author's bent—behavioral, physiological, cognitive—at best by selecting studies according to meet space limitations or at worst by blatantly misleading the reader with biased interpretations of the primary research.

Fortunately, there is a correction factor associated with these more conventional areas of psychology. Most psychologists are specialists in one or more of these subject matters. Therefore, they are more likely to dig into the primary sources in order to satisfy their own professional curiosity. This is not necessarily so with respect to parapsychology. Few psychologists specialize in this currently esoteric field.

The likelihood that a psychologist who reads one of the secondary sources (survey texts in ESP) will go on to read the primary sources (journal articles) is low. Two important reasons for this reluctance are (a) journals in parapsychology are not always available in college libraries, and (b) the reader may have already been prejudiced by the misleading interpretations contained in the secondary source. Thus, the vicious cycle continues.

Let us reinforce Child's (1985) plea for more attention to the primary sources of research in parapsychology. Perhaps we may break that vicious cycle.

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## Not so Anomalous Observations Question ESP in Dreams

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The *American Psychologist* is the best public vehicle for effectively reaching a majority of psychologists with diverse interests. So Child's (1985) critique of reviews critical to parapsychology illustrated on the positive the openness of *AP* reviewers and editors to provide a forum for Child, who felt that the Maimonides dream research had not come to the attention of many psychologists. The need for the critical reviews that Child attacked, however, is also illustrated. On the negative, the article bears several marks of pseudoscience (Radner & Radner, 1982), and in fact, is an excellent case study in the psychology of belief (Alcock, 1981; Singer & Benassi, 1981).

Child pleaded that the "procedures of science" require a serious consideration of the Maimonides research by psychologists, who (to their credit!), in contrast to scientists in all other disciplines, tend to consider ESP an impossibility (Wagner & Monnet, 1979). What Child proposed as an experimental hypothesis is that ESP influences dreams, or *general extrasensory perception* (GESP) may operate "without the mediation of an agent's thoughts or efforts" (p. 1220), or *Psi*, having "no identifiable source," transfers information "by channels not familiar to present scientific knowledge" (p. 1224). Some immeasurable influence by an unknown means with or without human agents supposedly results in some increment of knowledge that does not allow for any ordinary (non-anomalous) explanation. This is not a simple problem of "terminology." Rather, with no definable mechanisms in nature

or in normal science for ESP, GESP, or *Psi*, most scientists would stop without going further.

Child, however, asserted that the parapsychology critics have not been scientific. With reference to research by Bradley (1981, 1984), he suggested in ad hominem attacks that parapsychology critics, who are not primarily parapsychologists, have wrapped themselves in their "erroneous opinions" with the corresponding intensity of their belief in themselves as experts (p. 1229). C. E. M. Hansel's (1980) parsimonious explanations for the Maimonides research by sensory cueing and fraud were respectively labeled as "exaggerated" and "not entirely frank" (pp. 1225-1226). Alcock (1981, 1983) and Zusne and Jones (1982) were linked in an apparent conspiracy of "preconception and prejudice" (p. 1228). Those authors received the brunt of Child's attack. Although Hansel's (1980) criticisms have been available for many years, Neher (1980) and Marks and Kammann (1980) were also criticized by Child for not devoting space to the Maimonides research. Romm (1977) was attacked for her "ignorance" and "apparent willingness to fabricate falsehoods" (p. 1227). Instead, Child proposed that his analysis merits attention along with that of Akers (1984), both of whom are committed to the ESP hypothesis by belief and research.

Child asserted that familiarization with the Maimonides reports and other lines of experimentation would support the serious consideration of the ESP hypothesis. He asked psychologists to accept research that was "not properly analyzed at the time" and for which "the full original data are no longer available" (p. 1223). Consequently, we cannot inspect the data for "experimental flaws" or "other sources of error" as Child challenged (p. 1228). At another point, he called us to accept statistically significant results "attributable basically to just eight data points" (p. 1224). The opportunities for chance or deliberate fraud involving so few data points is a clear danger. The nearest replications of the Maimonides research somehow (by their number?) "add something" to the "promising technique" displayed by the Maimonides research—despite "the lack of significant results in the three systematic replications" (p. 1224). How many times do we have to accept the null hypothesis before rejecting the experimental ESP hypothesis? Further, these confirmations of the null hypothesis do not necessarily support "eventual replicability" as Child expected (p. 1225).

Indeed, there are striking parallels of the Maimonides research to other parapsy-

chological studies. The comparisons, however, would lead one to regard the Maimonides research with caution. Child deemphasized the statistical excess of hits over misses ( $p < .0001$ ), but he appealed to the fact that "there is some systematic—that is, nonrandom—source of anomalous resemblance of dreams to targets" (p. 1222). The source, however, need not be anomalous. In the Pearce-Pratt experiments (Rhine & Pratt, 1954), the excess of hits over misses was described by a bimodal distribution (Hansel, 1980, p. 114; Zusne & Jones, 1982, p. 378). The non-random distribution suggested a non-anomalous human intervention, most likely fraud.

In the Maimonides research, judges evaluated "every one of the 64 possible combinations of target and transcript" (p. 1227). However, there is also an obvious similarity of matching dreams to targets in the Maimonides research to studies of remote viewing (Targ & Puthoff, 1974, 1977). The identification of scenes by "remote viewing" is explained more parsimoniously by cues in the transcripts of described scenes presented to judges, and by the subject's subjective validation in post-hoc confirmatory visits of target sites (Marks & Kammann, 1980, pp. 12–41).

Uri Geller, instead of matching dreams to targets, matched his drawings to his "perception" of drawings inside envelopes. Geller's success is easily accounted for by ordinary peeking and cueing (Marks & Kammann, 1980, pp. 105–107; Randi, 1982b, pp. 39–60). The fact that in the Maimonides research there were a "great number and variety of personnel" does not make fraud "especially unlikely" as Child asserted, but to the contrary it increased the potential for fraud by only one or another of the participants—"experimenters, agents, percipients, and judges" (p. 1225). Psychologists, perhaps, more than others—except professional magicians (Randi, 1982a)—ought to be sensitive to issues of experimental control and bias in studies involving human subjects.

The Maimonides research need not be cloaked in anomaly, any more than unusual animal behaviors need be cloaked in "cognition" or "insight" rather than empirically validated principles of learning (Epstein, Kirshnit, Lanza, & Rubin, 1984; Terrace, 1979). Anomalous findings need not have mysterious sources—only perhaps less appealing sources than those found in normal science. The best scientific assessment of parapsychological research is still to be found in critical reviews (Hofstadter, 1982; Kurtz, 1985). Since the time of William James, the scientific case for parapsychology has not been convinc-

ing. Why should researchers devote any more time to such research?

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## Reply to Clemmer

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Clemmer (this issue, pp. 1173–1174) gives no indication that he is disturbed at the false accounts that Alcock (1981), Marks and Kammann (1980), and Romm (1977) have given of the Maimonides research, nor even that he has looked at the original research reports to check whether I was right that the accounts by those authors are false in extremely important ways. He seems to rely on a priori knowledge that there can be no anomalies and to be glad that psychologists are more likely than other scientists to adhere to this a priori knowledge, regardless of where this reliance leads them.

Clemmer's interpretation of the Maimonides results is based on analogies to defects he claims in three other pieces of parapsychological research. The bimodality he asserts for the Pearce-Pratt experiments seems to have no striking parallel in the Maimonides experiments. The defects he asserts to have characterized certain "remote viewing" and clairvoyance experiments are of types against which the Maimonides experiments were well protected by the experimental procedures. Thus Clemmer seems to follow in the tradition established by Alcock (1981) and Zusne and Jones (1982). Though he differs from them in not offering explicitly a false account of the dream experiments, the misrepresentations he implies are almost as bad.

The scientific tradition is complex and diverse. My respect for the role of general theory is a main reason that my commitment "to the ESP hypothesis" (p. 1173) does not go beyond a belief that the hypothesis merits serious exploration in hope of future discovery of the processes underlying the apparent anomalies. But facts are also important, and devotion to a theory to the point of disregarding or altering pertinent facts seems hardly compatible with the scientific tradition. A broad contribution of ESP experiments to psychology may come from their misrepresentation, if awareness of it alerts psychologists to the danger that excessive reliance on general theory may place our discipline at a disadvantage in the development of strikingly new areas of knowledge.